Contemporary Psychology

A Tour Inside the Onion

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This article deals with the author's concern for the quality of current psychological inquiry as an intellectual enterprise and for the effects of extreme specialization on the nature of the behavioral science disciplines. Worry is expressed for psychology's failure to confront fundamental metaphysical issues that make psychology, in Toulmin's phrase, a "would-be-discipline." In subsequent sections, attention is directed toward the role of narrative in science making and to the role of the rhetoric of inquiry in intellectual formulation. Attention is also directed toward the role of ideology in science, to the concept of order in scientific thinking, and to the essential properties of method in the human sciences. Finally, the writer issues a call for new institutional experiments that relate to the role of collaboration in intellectual work and to a need for new intellectual models in the social and behavioral sciences.

Info, info everywhere, but no one stops to think.
—Melvin Kranzberg, 1990, p. 265

Those who have become acquainted with me through the occasional essays (e.g., Bevan, 1982) that I have written for the American Psychologist over the last several decades will have marked me as a social activist of the more diffident sort. Meanwhile, I have preached with some depth of feeling about the importance of, to use George Miller's (1969) phrase, giving psychology away. I have argued vigorously that to contend that adherence to the scientific ideal requires indifference to the social implications of scientific outcomes is to succumb to a dangerous myth. The proposition that the learned professions must seek to direct their knowledge toward the public good is for me both a matter of quid pro quo and of ethical responsibility.

Those who have known me as a colleague in psychology know that I have a second, more serious, concern. I refer to my uneasiness about the quality of psychological inquiry as an intellectual enterprise. I worry about the possible consequences of what I see as the balkanization of psychology. I believe that ultimately the integrity of our scholarship must depend upon its being set in the real world of everyday experience; yet our present science-making strategies persist precisely in separating it from that domain. I despair at the increasingly frequent reports of careless and dishonest scientific practices that have not spared psychology from inclusion (see, e.g., Broad & Wade, 1982, Kohn, 1986).

How Modern Psychology Has Evolved

Put in their starkest form, my concerns may be described in the following way: Psychology faces a critical period in its development as a coherent body of knowledge. This crisis may be less a matter of shortfalls in substance and more a matter of flawed intellectual character. Certainly it derives, in part at least, from the sociology, psychology, and economics of the current academic enterprise. Although I am reluctantly brought to the truth of Sigmund Koch's (1976, 1981) contention that psychology is not a single coherent discipline but a fragmented collectivity of studies of varied cast, that insight in itself is not the source of my worry. My concern is, rather, that the character of psychology is increasingly manifest in the rapid proliferation of narrowly focused and compulsively insular camps, a proliferation that seemingly knows no limits. We persevere in looking at small questions instead of large ones and our view of the forest is forever obscured by the trees. Yet specialized knowledge derives its meaning, as Weintraub (1985) has observed, from the context of larger perspectives and questions. When it loses touch with that larger context, it loses its coherence and meaning. Indeed, as Weintraub has also observed, only an architectonic sense for the larger questions can inform us about how far we need go in our pursuit of more specialized knowledge.

I should make it clear that I am not disturbed by specialization in and of itself, for I have assumed that it

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is the natural outcome of the differentiation that derives in turn from evolution, whatever the latter's medium. What does disturb me is that we have thoroughly denigrated the currency of general ideas and set them aside as we got on with the practical logic of our specialisms. All of this has led to the dominance of the cult of the specialist.

Over the years I have found a disturbingly large proportion of the papers I have read to be trivial, some even contrived. The intellectual processes behind them too often have lacked clarity and crispness; manuscripts have been marked by a mindless and routine recitation of detail, more often submerging rather than elevating understanding. Preoccupation has frequently been at the level of data. Few writers have been concerned explicitly with fundamental metaphysical issues: Most have been narrowly focused, and the prevalent tone, despite the occasional theoretical fanfare, reflects a persistently tentative no-frills empiricism. It has been a long time since there was much talk among psychologists of grand explanatory schemes or of world views. Indeed, as Daniel Robinson (1976, 1982) has made clear, American psychology at its intellectual foundations remains at best a 19th-century enterprise. The language has changed, the methodology is more elaborate and refined, but the seminal problems remain the same in their general formulation. Yet, as I will argue later, we have not seriously confronted these problems for the greater part of this century.

What is said of substance must also be said of form. Both the model of the science-making process and its affiliated metaphysics remain simplistic, if not simple-minded. Although much has been made of the alleged cognitive revolution, we continue to be stubbornly reductionistic and mechanistic in the way that physics was mechanistic before the advent of relativity theory. Look carefully and you will still see a machine—this time a versatile computer—inside the ghost. We are still, as Stephen Toulmin (1972) pointed out, a "would-be discipline" (p. 386).

I suspect that talks like this are being given more often these days, although certainly they are nothing new. They have been occurring as long as I have been in psychology. They are part of a tradition of self-doubt that Henry May (1983), the distinguished intellectual historian, said has been evident in American intellectual community from its inception. Its origins are several but the tradition derives, if May is right, mostly from the Puritan spirit, a set of values that was an inevitable part of the American psychology of 100 years ago. Despite the secularization of the American intellectual community, its ethnic diversification, and the radical social changes that have taken place in it as part of the larger society, particularly in the last several decades, the Puritan spirit is still very much alive. Articulate beliefs change with the passage of time: Emotional attitudes endure much longer. What we are now experiencing is one of those recurrent waves of intellectual soul searching that seem necessary for our balance and that speak to the fundamental nature of our national as well as our professional character.

What We Say, What We Believe, and What We Do as Scientists

When one examines in a first-hand way how scientists actually think and behave, one quickly understands that formal accounts of the process and what actually takes place are very poorly matched. Real-life scientific problem solving does not really happen according to the canonical script. It involves impressive amounts of tacit knowledge that never surface in formal reports, and is a far richer, more complex, and more intuitive process than any textbook account ever suggests. Thus, scientific methodology proves not to be qualitatively distinct from other methodologies and scientific knowledge nor so very different from other knowledge in how it is acquired.

Good science requires good scientists, by which I mean moral, honest, hard-working people who will look at what they are doing from all angles, test their intuitions, check their insights, debate with their colleagues, paying their nickles, and take their chances. There are no formulas that will guarantee success. All of which is not to say that science and the nonscientific disciplines are identical, but it is to say that both are creations of the human mind. As Nelson Goodman (1984) put it, what takes place in science, as in art and in everyday life as well, "involves the use—the interpretation, application, invention, revision—of symbol systems" (p. 152). The quality of the science that we have available to us at any particular time is as much a reflection of the personal qualities of the science maker as it is of the procedural formalities through which that particular science was made. Certainly logical thought and a commitment to evenhandedness are important instruments in the advancement of scientific knowledge; but to believe that they are the only factors at work is to be coopted by a myth.

The logical structure of scientific knowledge is itself imposed as much by the hindsight of the observer as by the properties of nature observed. Moreover, a persuasive case can be made that knowledge grows, at least in part, out of the processes that shape the narrative through which scientific outcomes must be reported and summarized. The conventions of scientific report writing rigidly require that every aspect of a scientific study be described as if it conforms to the idealized method of doing research. Thus the false leads, intuitive gropings, fortunate and the unfortunate guesses, tinkering with procedure, and artful shaping that are part of every study, successful or not, go unmentioned. So, too, do the researcher's assumptions and perspectives—biases, if you will—that are governed by the researcher's world view and location in time and place. Indeed, I would insist that there has never been a scientist who, in reporting the outcomes of his or her work, has not first selected and arranged the elements of his or her account to make a persuasive case for its being reported. Thus, the preparation of the scientific report is an exercise in rhetoric shaped as much by ingrained acts of persuasion as it is by the tenets of ideology.

Hayden White, the distinguished historiographer, argued in his much debated study of the historical imag-
ination (White, 1973) that historians, in their treatment of the data of history, select both descriptive and explanatory modes that reflect their attitudes toward the human condition (see also Canary & Kozicki, 1981; Lottinville, 1976, pp. 3–18). William Kessen (1983) and Jerome Kagan (1986), following White, saw in the character of present-day developmental psychology the deepest assumptions of the American social tradition, and stressed the importance to the working scientist of an understanding of this fact. This is, in part, the point I have been trying to make, but I would go farther to insist that no intellectual activity, science included, is ever free from the shaping force of one particular ideology or another.

Against such a background it is worth noting that there is a currently vigorous debate over the failure of the philosophy of modernity, which has science at its core, although that philosophy has clearly served science and its antecedents well since the days of Francis Bacon. (For a good short comment on the “trial” of modernity see Kolakowski, 1986; Whitney, 1986, has written an excellent monograph on Bacon and modernity; Toulmin, 1990, has written an interesting history of the concept of modernity.) Even I have taken to feeling increasingly comfortable with the tenets of postmodernism. Robert Nisbet (1980), the conservative sociological theorist and, for my money, the most impressive representative of that community, in a book-length examination of the idea of progress, admitted that after 250 years of usefulness, this concept, central to the philosophy of modernity, was in trouble (see also Nisbet, 1988). Certainly the record has not always been an unblemished one for the consequences of a belief in progress. After our 20th-century experience with the powerful totalitarianisms of both the left and the right, and with the coincident association of science and technology with nuclear warfare, the degradation of the physical environment, and the exploitation of humankind, we have rapidly lost faith in the dogma of progress as an unadulterated good.

In a related vein, to insist on the objectivity of a science in terms of its separateness from the life experiences, intentions, values, and world views of the persons who create that science is to deny its fundamental character as a human activity. This becomes increasingly clear as we gather a body of critical historical scholarship in psychology.

Increasingly, people are coming to believe that the subject matter of the human and social sciences is fundamentally different from that of the natural sciences (Winkler, 1985, p. 6). As Paul Starr said, “A molecule is not going to lie to you. It is not going to misunderstand you. But people do, and that makes studying molecules and men [and women] different” (cited in Winkler, p. 6). Furthermore, when one recognizes that science is influenced by its ideological context, one must raise serious questions of an epistemological nature. For psychology and the other social sciences, these chiefly concern the appropriateness of the epistemology identified with the intellectual tradition that we have borrowed without question from classical physical science. Some among us who care about these matters now see our 100-year-old intellectual commitment to be wanting. To set aside this orthodox epistemology is either to cut ourselves adrift without a methodological anchor or to recognize that there are multiple epistemological alternatives, some presumably more appropriate than others for the human sciences.

As I close this section, I will make two additional points: First, in emphasizing the important influence exercised by ideology, I am not saying that all of the intellectual issues resident in science are ideological. (For a good discussion of ideology at the core of the behavioral and social sciences, see Manicas, 1987.) Second, in expressing my own pessimism about psychology in its relationship to the ideological tradition of 19th-century physical science, I am not seeking to impose some particular alternative tradition on others. Rather, what I have been attempting to say is that the long course of history has made clear that all secular communities, science as much as any other, are finite and flawed; that reality is complex; and that there are no given, cast-in-stone strategies for confronting and coming to an understanding of reality. In the social and behavioral sciences our ideological fate was finally sealed when, during World War II, the federal government increased its support of science. This led to the creation of research universities (a good exposition of the nature of the research university has been written by Rosovsky, 1990). Not to be left behind when the money was being distributed, the behavioral and social sciences jumped on the bandwagon, and this more than anything else made for our lack of critical insight when it came to scientific ideology (see Klauser & Lidz, 1986). For most of us—scientists and laypersons alike—uncertainty is fundamentally intolerable and the uncritical assumption of a metaphysical posture provides a welcome escape from our anxieties. Unfortunately, that is a luxury we cannot afford. A constant and vigilant skepticism about the intellectual structure of our discipline is an essential prerequisite if our hopes for its future are ever to be realized.

Every Writer Tells a Story

As I noted earlier, narrative form is a primary cognitive instrument for making comprehensible an ensemble of human events regardless of their setting. How a narrative is constructed will directly influence what is conveyed and how. This certainly bears on the issue of the nature of knowledge, regardless of its setting—in science, the humanistic fields, or the arts. The cognitive function of narrative form consists of more than relating a series of events. It brings together an ensemble of interrelationships of many different kinds as a unitary whole. Whether in science or elsewhere, the data themselves simply do not determine what the ultimate construction will be. According to Jerome Bruner (1986), scientists build a world view characterized by an invariance of things and events across transformations in their life experiences. Humanists, in contrast, make a world that is understood in terms of the particular shifting requirements for living in it.
One is context-independent, the other context-sensitive. But as I have argued earlier, to the extent that scientists are involved in building and testing models of their assumptive worlds, they are engaged in the same kinds of activities that preoccupy humanists and artists. When we psychologists, perennially anxious about our identity as scientists, insist that science making is totally different in kind from other exercises of the human intellect, we are perpetuating a false dogma, the effect of which on the future of the field can only be pernicious.

**Good Science Is Good Conversation**

Behind all scientific studies there is not only the drive to understand but the compulsion to persuade. All scholarship, including science, uses argument, and argument uses rhetoric. One of the more fascinating developments in the field of critical scholarship in recent years has been a shift in interest away from the logic of inquiry to the rhetoric of inquiry—that is, scrutiny of the scholarly process from the perspective of how scholars converse and seek to convince. From the perspective of argument, scholarship takes on a form substantially different from that suggested, for example, by the logical positivism that governed the behavioral sciences for the last 40 years or more.

The approach through rhetoric does not deny the legitimacy of methodological concerns, but it does insist that methodology be delineated in terms of actual research praxis rather than an abstract philosophy of science. It reaches beyond official methodologies to examine how research is actually conducted. In so doing, it emphasizes the interdependence of inquiry and communication. As Nelson, Magil, and McClosky (1987) noted, a critical study of rhetoric has helped to make clear that diverse fields of scholarship share the grammar of their culture to a far greater extent than their practitioners have realized.

How does this line of thinking relate to my earlier observations? I want to argue that the resolution of the intellectual crises that we must confront requires that we go beyond the official philosophies of science, beyond the blinkered use of prescribed methodology, beyond our all-too-common science by the numbers, to an understanding of the psychology and sociology of the science-making process itself. Both the process of inquiry and the process of communicating its outcomes involve constructing worlds, telling stories, and advancing arguments. Behind the stories and arguments are the processes of rhetoric and logic. Let me now repeat an earlier point: Behind the worlds we construct, coloring both our logic and our rhetoric, are the ideologies that give our world views their dominant cast. Such ideologies are complex and not easily analyzed. Moreover, as Kenneth Minogue (1985) said, they are like sand at a picnic—they get into everything. As explanatory constructions, they represent a kind of free play of the intellect that invades the several intellectual planes of self-understanding. As forms of human thought, ideologies permeate virtually every aspect of our mental life, including our science. We ignore them at our intellectual, social, and personal peril.

**Science in a New Ideological Age?**

In noting psychology's dependence on the ideological tradition of the physical sciences of the 19th century, I said that I would not impose any particular ideological alternative. But behind what I have been saying is a testimony to personal change. Certainly my own ideological stance now includes elements that differ from those on which I was professionally reared and that constituted official doctrine during the days of my scientific training. I have crept up on this matter softly because I am trying to avoid using labels. I have said that I have personally come to feel comfortable with the tenets of postmodernism although, like Leszek Kolakowski (1986); I am not confident I understand precisely what modernism is, or that I can provide an integrated summary of modernism as a philosophy. Here, however, are some bits and pieces: Modernism is said to emphasize the advance of the present over the past—"progress." This advance results from the play of the human intellect, its principal instrument being science and science's attendant technologies. In modernist science, value is segregated from fact; reason prevails over revelation; there is an assumed world beyond experience; and this objective world is ultimate and functions according to natural laws. Erwin Schrodinger (1967) summarized the philosophy of scientific modernism with two fundamental tenets: (a) the belief that nature is objectifiable (i.e., that it has an existence independent of the observer); and (b) the belief that nature is knowable (i.e., that it is accessible to the operations of the human mind).

My diffidence in the matter of ideological choice stems from my instinct to revise rather than to reject. Modernism has been a productive philosophy for an impressively long time, and there is much about modernism that defines the structure of my personal life, although I have been greatly influenced by postpositivist scholars like Michael Polanyi and Stephen Toulmin. However, the key consideration, I think, is that one can do good science regardless of one's religion. Thus if you want to understand what effective science making is about, don't listen to what creative scientists say about their formal belief systems. Watch what they do. When they engage in good, effective science making they don't, as a rule, reflect on their presuppositions; they engage in a practical art form in which their decisions are motivated by the requirements of particular problem solving.

Perhaps I can best illuminate my own particular intellectual odyssey by drawing on Evelyn Fox Keller's (1985) discussion of the assumed centrality of natural law in modernist science. First, she pointed out that laws in the causal, deterministic sense apply only under the very limited conditions of Newtonian physics. In contrast, with the advent of quantum mechanics the subject-object dichotomy collapsed and the operation of laws of the classical sort no longer applied. Indeed, what we mean by the term law has itself changed with the passage of
time. Scientific laws may now also be statistical, phenomenological, or simply the rules of the game. The lessons of relativity theory, quantum mechanics, and molecular biology thus prompted the search for an alternative. Keller found this in the concept of order, which she characterized as wider than law and free from its coercive, hierarchical, and centralizing implications. Order includes patterns of organization that can be spontaneous, self-generated, or externally imposed. Thus the kinds of order generated by law are only a subset of a larger class of observable or apprehensible regularities, rhythms, and patterns.

Adoption of the concept of order in place of law, Keller suggested, could significantly change what science is understood to be. By looking to the broader varieties of order found in nature, we might shift our attention away from the hierarchical models of simple, relatively static systems toward more global and interactive models of complex dynamic systems. Nature, then, is neither law-bound nor chaotic and unruly. From Keller’s perspective, molecular biology, for example, is best seen in terms of a nature that is orderly in its complexity rather than lawful in its simplicity. Keller went on to say that because science is an open system, several conditions obtain: Nature transcends our laws; the practice of science transcends its own prescriptions; and the making of science involves the interplay of theory, ideology, and practice.

Where does all this leave us? Is there a postmodern life for psychology after fragmentation? I believe there is. And, if we are to ensure it, there are certain things we must get on with. In general, Keller’s reconceptualization must be applied to the human sciences. We would do well to explore new models, to approach the domain of method with a new set of attitudes, and to experiment with new and different operational styles. Our strategies must relate both to the intellectual styles of the individual scholar and to the institutional structure of the enterprise. As part of this, we must give careful attention to the role of language in the making of science. Above all, I believe we would do well to reacquaint ourselves with the great philosophical themes that were once the preoccupation of the scientists—Max Born’s “iridescent fancies of metaphysics” (cited in Toulmin, 1972, p. 294)—as a stimulus for clarifying and experiencing new insights.

Models and Methods

When I suggest that we should look to new models, I am not prepared to say precisely what those models should be. However, I believe it essential that they should recognize both the great complexity and dynamic quality of the phenomena with which psychologists deal. They should also be representative; that is, they should allow for successful generalization or translation to settings in which the hands-on, live phenomena that are of principal interest to us occur.

In making these comments on models and methods I would like to call special attention to Complex Systems Theory or, as it is alternatively called, nonlinear dynamics. Complex Systems Theory is derived from a 1948 paper by Warren Weaver. There is a growing interest on the part of applied mathematicians (e.g., Gleick, 1987; West, 1985) in the potential usefulness of such a mathematics for the study of psychological and social processes. From their point of view, the stability of the patterns that characterize such processes is seen to be a consequence of their nonlinearity. This implies, in turn, that the linear analyses that have been used for so long are simply inappropriate.

When it comes to a methodology for the human sciences, I would urge first that we give it more self-centered attention, notably in the context of problem solving in specific concrete situations. In a 1959 address to the Eastern Psychological Association, Sigmund Koch made a plea for what he called an indigenous methodology that grows out of an understanding of the properties of the phenomena with which we deal and the settings of which they are a part—a methodology that is, in important and essential ways, isomorphic with those properties. It is better to speak of methodologies in the plural, for it is important to assume a far less rigid, more pragmatic view toward what scientists may or may not do. The great disservice that results from the generic methodology associated with modernism lies in its stamping some procedures as scientific and the others as unscientific; some as legitimate and others not. (The narrowly conceived quarantine against introspection, in effect for so many years, is a useful example of what I have in mind here.) It would do us all good to loosen up and look around, not only to our closer relatives in the biological sciences and in the social sciences, but to the humanities as well. Koch (e.g., 1961) has counseled for many years that we would benefit greatly from the kind of sensitivity to the subtleties of individual experience that is characteristic of insightful inquiry in the humanities.

I will translate these attitudes toward methodology into a few short declarative sentences of advice. First of all, ask yourself what the essential questions to be answered are, and then ask yourself what you have to do to reach answers that will convince you and others of the validity of the ideas behind them. Second, be wary of rule-bound methodology. Use any method with a full understanding of what it does for you but also what constraints it may place on you, and whether it violates assumptions about the phenomena that you are studying. Free yourself of the worry that you are behaving badly if you don’t use officially certified scientific methodology. Rule-bound methodology frequently degenerates into methodology by formula. Formalized methodology claims priority in science, but in doing so, as McClosky (1986) observed, it involves itself in a contradiction; namely, it pretends to know how to achieve knowledge before that knowledge has actually been achieved. “Life,” McClosky said “is not so easy” (1986, p. 53). Rule-bound methodology is one ingredient in what might be best described as “centralized, bureaucratized, methodized science,” to again borrow from McClosky (1986, p. 53). That is the most direct road to shallow science by the numbers. My third corollary is this: Be mindful of the potential value of methodological pluralism. Several heads working dif-
frequently are usually better than one. Finally, as in any other aspect of creative scholarship, there is no substitute for careful critical thought in connection with the selection or invention of methods for scholarly inquiry.

Having unburdened myself about the proper general attitude toward method, I do not hesitate to prescribe a program of improvement. We must go about change in three ways. First, we must give attention to activities that will ensure greater depth in the use of methods that have already been proved useful for the scholarly domain. In the current atmosphere for both conducting research and receiving research training, psychologists mainly acquire an acquaintance with existing technical procedures, and far less attention is directed either to the broad intellectual content of the field as it informs method or to the logic and rhetoric of the investigational process itself. Second, we must explore the possible usefulness of methods developed in other disciplines. At present there is general ferment in the larger scientific community about the value of such adaptations. I refer not only to what is happening in molecular genetics, which is expanding at an almost incomprehensible rate, but to older fields such as population genetics and behavioral genetics, which can provide methodological lessons for us. In any event, this ferment seems to be depressingly slow in reaching psychology. Third, consistent with Koch's earlier call, we need to vigorously foster the creation of entirely new methodologies. Again, this must go beyond the mere invention of new technologies to a serious examination of our field's underlying rationale and logic. We have seen how new methodologies have made possible significant advances in other fields—the most dramatic example is the reorientation of biology following the emergence of molecular biology—and there is a sense of urgency about a comparable revolution in our field.

**The Social Construction of Scientific Outcomes**

I would suggest that we need new operational strategies. These must include new organizational arrangements for research that will accomplish several things. For one thing, they would help to deemphasize the unhealthy scramble for stardom that preoccupies the American scholarly community. I suspect that there already are forces in operation that will work toward that end, principally the diminution of federal and other research funding. In the face of such reality, unless we develop social and psychological mechanisms to accommodate it, the scientific community at best will be bitter and frustrated. What is called for are new institutional arrangements that will get around the limitations of the structures that currently carry the burden of our activity as a profession. The days of the mom-and-pop shop in the research community are fast coming to an end. I cannot tell you with any great precision what the new arrangements will be like, but I believe that, whatever their form, if they are to work, they will have to serve the psychological ends of greater community.

My colleagues at the John D. and Catherine T. MacArthur Foundation have slipped into the use of an acronym to cover this guess about the future. We refer to it as the three C's—communication, collaboration, and collegiality. We agree with Blalock (1984) that neither university departments as they are now constituted, nor the national scholarly and professional societies as they now operate, provide a climate suitable for what we think must evolve. None of us can think of a university department of psychology in which there are more than a very small handful of faculty members who engage in genuine intellectual exchange with one another. As for the professional and scientific associations, political agitation within them usually works to inhibit their taking forceful, coherent intellectual positions on anything. Look at the American Psychological Association (APA). It generates new divisions faster than one can find names for them, putting me in mind of an apocryphal story about David Starr Jordan, the distinguished ichthyologist and early president of Stanford. When he was accused of not knowing his faculty he would explain that every time he learned the name of a faculty member he forgot the name of a fish. That is my situation with APA divisions and other special interest groups: Every time I learn the name of a new one I forget the name of an older one.

New institutional experiments are needed. More than ever before, they must somehow be directed toward integrating themes and strategies in the face of an increasingly complex web of intellectual interests and a corresponding change in scholarly diversity. We shall have to find ways of more effectively sharing a rapidly growing knowledge base, of exchanging widely diverse intellectual skills, and, most important of all, of working together at the level of ideas and the ways they are best developed. If we are successful, I believe there will be a number of changes for the better. As I suggested earlier, more critical attention will need to be given to methodological issues as they relate to the properties of the specific problem being attacked. There also will be more systematic concern for the development of general intellectual resources to serve the field. I am hopeful that our research agenda will be broader in perspective, more comprehensive, longer ranging in commitment, and more insightful when it comes to intellectual priorities. I fervently hope that we will soon see the short-range, excessively narrow, often trivial, mostly routine research projects that now dominate the landscape of our research enterprise give way to bigger and better gambles.

New organizational strategies will come about neither easily nor rapidly. Researchers and institutions are conservative. If anything, their current anxiety over what is happening in the world of sponsors and grant funds is likely to make them even more resistant to change than before. It will take time and great patience for researchers to learn to talk, think, and act together. Can it be done? For several reasons, I must say emphatically that I think it can be. First, collaborative research strategies have worked for some years in such fields as the experimental physics of subatomic particles, in radioastronomy, and even in some areas of theoretical physics. Second, because
we are carrying out certain institutional experiments at the MacArthur Foundation that make us very hopeful (Bevan, 1989; Prager, 1990). I refer to the MacArthur Mental Health Research Networks and other programs designed to promote collaboration in research on topics important to mental health and human development. There are now some 12 mental health research networks; of these programs, 4 deal with human development, 3 focus on the processes underlying maladaptive behavior, 3 relate to the study of the mind in context, and 2 are concerned with the field of mental health policy. Each consists of between 10 and 15 individually selected, well-established investigators, all oriented around a single topic. Each network holds a seminar about every six weeks, continually plans activities aimed at facilitating the work of the network as a single entity, and seeks ways of facilitating collaboration between its researchers and those of other networks.

Is there anything other than the usual coming out of all of this? Of course, in our own appraisals, we may be seeing what we want to see. However, after some hard-won experience in learning to communicate across areas, disciplines, and separate intellectual communities, we think researchers are being more constructively critical of what they are doing. We believe they are asking questions that get at the fundamentals of quality science making. We see them pooling ideas, developing new angles for looking at things, putting things in broader perspective, and asking in more self-critical terms about what is important and what is not. They are looking beyond the nice things of design to the assessment of relative importance, from matters of data to the human and social meanings of these particular endeavors and, perhaps, of the enterprise as a whole.

Psychology and a Unified World View

Perhaps the most exciting aspect of all of the recent ferment in the scientific community has been an emerging redefinition of the scientist’s role, from being an arm’s-length observer of nature to a participant in natural events. With this new perspective a blurring of the institutional lines has begun to occur between pure and applied science and between knowledge and practice. The drive toward understanding has also begun to be tempered by a commitment to ethical concerns. What took place in the physical and biological sciences in the 1970s can be hoped for in the human sciences in the 1990s.

Certainly a further element in my optimism is Stephen Toulmin’s (1982) contention that the advent of the postmodern period and the broader, more flexible perspective associated with it can stimulates a return to the great cosmological questions that have fascinated the more imaginative and adventurous minds in the past. However, if Toulmin is right, it will be a cosmology with a difference. It will incorporate human agents into the natural world and put back together the whole that analytical science, over the past 300 years, has rent asunder. It will be a cosmology in which order and design will join evolution and adaptation as characterizing concepts and in which psychology must inevitably assume a significant role. My growing discontent with psychology over the past several decades stems from, among other things, my perception that too many psychologists hug the intellectual shoreline and are content to paddle quietly in their own small ponds. We live in a coherent world, although one of never-ending complexity. The big questions about it will not be answered if scholars simply attend to the comfortable little questions, no matter how important these latter exercises may in themselves seem to be. Robert Browning, the great 19th-century English poet laureate, once described his interests as lying on the “dangerous edge of things.” His phrase is an apt one. Living at the dangerous edge of things is what psychologists should do more of as they participate in the new postmodern world.

Robert Bellah and his colleagues, in their impressive book, Habits of the Heart (Bellah, Madsen, Sullivan, Swidler, & Tipton, 1985), displayed deep conviction about the things I have been trying to say in this article. They pointed out, as many others have, that the prestige of the natural sciences as the model for understanding the world, and the uncritical faith that progress in the sciences would inevitably bring with it solutions to the world’s pressing problems, have together obscured the loss of the unity and ethical meaning of the scholarly enterprise. They rejected the widespread assumption that the demands of specialized scholarship necessarily force scholars to give up the roles of general citizenship. Specialization requires integration; the two are not mutually exclusive. Moreover, it is essential to remember that the professions, including the scholarly professions, were once a calling, and callings imply public responsibility. Thus, we should never simply be specialists. We must see our science, in part at least, as social or public philosophy. A science with such philosophical roots must have a vision of society as a whole and should understand, in turn, that it must, in some sense, transcend disciplinary boundaries altogether. We must become part of the tradition of social self-understanding and must view the present in the context of the past. By scrutinizing values as well as facts, we should be better able to confront the hard questions that too often have been avoided in the scientific and scientific past.

Nothing that Bellah et al. (1985) said, or that I have said, implies that a science that looks beyond its own narrow disciplinary lines must be monolithic. A sensitive rhetoric of inquiry sees good science as good conversation, a dialogue about the meaning and value of our common life experience. One of the important features of the emerging postmodern ambience is that, in the face of a dominant and self-conscious separatism, there have been movements in the scientific community that have sought and continue to seek dialogue with the broader public about matters that concern all citizens—dialogue about the common weal.

Rounding Out

With this recognition of science as public philosophy I have come full circle back to the way that I ended my
contribution to the 1983 Centennial Symposium honoring G. Stanley Hall's founding of the Johns Hopkins Psychological Laboratory. On that occasion I said,

When we look at ourselves as psychologists in 1983, we must certainly recognize that as we have advanced our academic enterprise we have also accelerated specialization and fostered a technological frame of mind at the expense of one that is broadly historical and philosophical. We have in point of fact denigrated the currency of general ideas. If then, this constitutes, as I have suggested, an unhealthy imbalance, it cannot be rectified by the recognition of that fact alone but only by a fierce and constant will to locate our specialized knowledge and insight, whatever it may be, within some significantly broader philosophical, historical, and—may I add?—moral perspective. (Bevan, 1986, p. 393)

In trying to devise a title to bring this rather unsteady interpretive exercise together, I ruminaged through my mind for an appropriately impressive literary allusion. I recalled a phrase in a poem by Vernon Watkins (1967), a greatly under-appreciated Welsh contemporary of Dylan Thomas. In the opening stanza of Good Friday, the poet described the uncertain lengthening of the days of early spring by a couplet: "The cockerel step by which the light shortened the sleep of earth and night" (pp. 78–79).

The phrase, the cockerel step, strikes me as an accurate characterization of the way we psychologists have behaved over much of our history. It is the step that marks the young, insecure rooster, as distinguished from the established cock of the walk. It begins with great flourish but ends, after obvious hesitation, just about where it began. The confidence that was in evidence only moments earlier has been completely dissipated. I was going to say that as we faced the realities of the larger, more complex postmodern world, I would fervently hope that our early steps would be the cockpit steps of an intellectual enterprise that collectively knows what it is and where it is going. But then I was reminded of another metaphor. Howard Nemerov (1984) in a poem entitled Inside the Onion, described what one experiences when one takes the seemingly unitary sphere and cuts it in cross-section.

Slicing the sphere in planes you map inside
The secret sections filled up with the forms
That gave us mind, free-hand asymmetries
Perfecting for us the beautiful exact
That mathematics may approximate
And clue us into but may never mate
Exactly...

But making the onion
One and the same through the in and out.

Our science will hit its stride when we understand that our reality is formidably complex, dominated by asymmetries, and forever challenged by the unpredicted. We will be in command of that reality when we have created strategies that will allow us to deal in a systematic way with what now we can only see as unsystematic. I cannot say whether or not Complex Systems Theory, for example, holds the key. If not, then eventually something else will. Until we achieve an approach that is suitably conceptual, methodological, and technological we are going to continue to write natural history rather than make truly effective science. There is no future in chasing butterflies with the model of 18th- and 19th-century physical science.

REFERENCES


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